The second section, on muscles, has not been changed. Here Fuerbringer's and de Man's nomenclature might, with great advantage, have been added to, or rather given instead of, many of the antiquated synonyms of Zenker and Dugès. These, however, are points of minor importance, and are, after all, matters of opinion.

The account of the central nervous system, and that of the sympathetic system, are quite new. The same applies to the heart. Some excellent figures illustrating the anatomy of the heart have been added, or have taken the place of old illustrations. The account of the arterial system remains practically unaltered, but many additions have been made to the venous system, and the description of the lymphatics has been rearranged. Much labour has been devoted to the histology of the alimentary canal and its appendages, to a great extent based upon original research made by the translator himself. A summary tabulation of the researches on the structure of the intestinal epithelium will be found on pp. 288–290. Section VI. is devoted to the respiratory organs and the neighbouring glands. A second carefully-finished and coloured plate contains many histological drawings, mostly original, of lungs, liver, and kidneys. The histological account of the thyreoid and thymus is almost entirely new, and a pair of lymphatic glands in the hyoid region, hitherto mentioned by Tolldt only, have been rediscovered and have been described as tonsils.

"A very large number of preparations have been made to investigate the vessels and uriniferous tubes of the kidneys; and the description of the remaining organs of this section (genital organs, adrenals, and fat-bodies) has received large additions from recent publications." The description of the minute structure of the fat-bodies, with illustrations, is a new and original contribution. The eighth, or last section, treats of the skin and sense-organs. The latter were treated somewhat summarily, and meagrely illustrated, in the original edition. This defect has been amended by so many new illustrations, and by the addition to the text of the results of so much recent research, that the whole section has assumed a completely new aspect. Especially may be mentioned the structure and distribution of the peculiar tegumentary papillæ and other tactile organs. The various other sense-organs, especially the ear, and above all the eye, have received much painstaking attentention, and have been amply illustrated by copies from the works of the most recent writers.

Of the 227 woodcuts we may say that they have been so carefully cut, and come out so clearly on the good paper, that the blue and red colours used for the vessels and muscles in the original edition are not at all missed. It would be going far beyond the scope of a general review to point out all the important additions and emendations which the new book contains (by the way, clearly indicated by square brackets), nor would it be fair to search for mistakes—of which, after all, there seem to be remarkably few. Those who use the work, whether for the sake of the many hundreds of references to the literature, or in order to be guided in the dissection necessary for a delicate experiment, will soon find that Dr. Haslam has greatly improved a book which was already good.

H. G.

OUR BOOK SHELF.

Syllabus of Elementary Dynamics. Part I. Linear Dynamics; with an Appendix on the Meanings of the Symbols in Physical Equations. (London: Macmillan and Co., 1890.)

THIS is a small pamphlet in which the author defines the terms usually found in works on dynamics. When dealing with measurements of quantities, he adopts an appropriate series of capital letters for specified units, and another set where the units are left undefined; thus equations containing the latter class of symbols possess more generality. Other units of higher dimensions are represented by capitals which are over-marked. Thus an acceleration of 10 feet per second per second is written as $10\ddot{F}$. These are very useful for the author's purpose, though it requires no little time to become used to them. A few formulæ and results are obtained in connection with falling bodies, energy, and centre of mass.

Where quantities are represented by certain units and multipliers, it becomes necessary in every case to state any existing relations between the units employed. Instead of being under any such necessity, many physicists prefer to regard these multipliers as completely representing the quantities themselves. The advantages of such a system are discussed in the appendix, and a series of examples worked out in parallel columns, showing the advantage of the one or the other of the two methods suggested.

G. A. B.

Organic Evolution, as the Result of the Inheritance of Acquired Characters according to the Laws of Organic Growth. By Dr. G. H. Theodor Eimer. Translated by J. T. Cunningham, M.A., F.R.S.E. (London: Macmillan and Co., 1890.)

THE work of which this is a translation we have already reviewed. The only additional matter contributed by the translator seems to be an ill-advised reference to NATURE in the preface.

LETTERS TO THE EDITOR.

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Bison not Aurochs.

IN his excellent article on the extermination of the American bison, "R. L." remarks (NATURE, p. 11) on the transatlantic practice of miscalling that animal a "buffalo"; but on the next page he writes of "its European congener the Lithuanian aurochs." This is to perpetuate a common error at least as bad. The "aurochs" (= ox of yore), Latinized by Cæsar in the form *urus*, is or was the *Bos primigenius* or *B. urus* of scientific nomenclature. It is wholly by mistake that in its extinction as a wild animal its ancient name was transferred to the bison, or *Zubr*. I would invite "R. L." to turn to the word "Bison" in Dr. Murray's "New English Dictionary," where he will find a reference to an article "*Wisunt*" in Schade's "Altdeutsches Wörterbuch," which ought to settle the question. I only wish one could ascertain to what animal the name "buffalo" strictly belongs. There unfortunately Dr. Murray does not help us.

ALFRED NEWTON.

Magdalene College, Cambridge, May 4.

Unstable Adjustments as affected by Isolation.

IN a brief passage in his very suggestive volume on "Darwinism," Mr. Wallace refers to a principle which seems to me to be worthy of much wider application than he has given. It is a key which requires only a little filing to prepare it for unlocking some difficult problems in divergent evolution. Speaking of the infertility of crosses, he says:—"It appears as if

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fertility depended on such delicate adjustment of the male and female elements to each other that, unless constantly kept up by the preservation of the most fertile individuals, sterility is always ready to arise. . . . So long as a species remains undivided, and in occupation of a continuous area, its fertility is kept up by natural selection; but the moment it becomes separated, either by geographical or selective isolation or by diversity of station or of habits, then, while each portion must be kept fertile *inter se*, there is nothing to prevent infertility arising between the two separated portions" (p. 184). Here is an application of the principle of segregation, or of like to like in groups that do not cross, in which indiscriminate separation is followed by increasing divergence in the different portions, not because they are exposed to different environments, not because there is any advantage in such divergence, not because there is any need that the function should be performed more perfectly in one portion than in the other, but because intergeneration, which is the principle by which correspondence of function is secured, has been suspended for some generations; and, in the absence of intergeneration, neither natural selection nor any other principle is capable of preserving complete correspondence. In organisms that reproduce sexually, the causes of divergence are many, though they may all be classed as causes of segregation, while the causes of correspondence with variation, whether of functions or of structures, are causes of intergeneration between partial segregations. If the environments surrounding the isolated portions are the same, the *use* of the environment, and therefore the forms of natural selection, may become divergent; if the use continues unchanged, some *useless divergence in the method of securing the use* may appear. Or, if all the relations to the environment, whether useful or useless, remain unchanged, "the adjustment of the male and female elements to each other" are liable to become slightly divergent, producing *mutual infertility*. Or the *preference of the sexes* for certain shades or arrangements of colour in their mates may become slightly different. Through some slight difference in the hereditary elements, distributed in each separated portion at the first, one or all of these causes of accumulated divergence may be introduced. I think it is evident that we have here a general principle, which is as applicable to a wide range of divergences as it is to the divergence that produces mutual infertility and sterility.

The context shows that the prominent idea in Mr. Wallace's mind was divergence in the adjustment of the male and female elements through correlation with "some diversity of form or colour," resulting from divergent forms of natural selection, that had been induced by exposure to "somewhat different conditions of life." But if the reasoning is correct in the sentences I have quoted, it gives an explanation of similar divergences when the separated portions are exposed to the same environment, *and where there is no possible advantage to be gained by divergence*. This is one of the principles I have used in the explanation of the divergences of Sandwich Island land mollusks; and I think that in the earlier stages of the development of infertility between allied forms it is often the only explanation that is applicable. It should, however, be remembered that, for divergence of this kind, it is not always necessary that the isolation should be either complete or very long continued, and that, when the forms that are not fully fertile with each other meet and more or less commingle, there is, through the very laws of propagation, without any aid from natural selection, a constant increase in the ratio of the pure breeds to the mongrels, and an accumulating intensity in the segregative instincts and the physiological incompatibilities. As this point has been fully discussed in my paper on "Divergent Evolution," I do not need to enlarge on it here (see Linn. Soc. Journ., Zoology, vol. xx. pp. 246–72).

There is, however, another phase of the subject which is indicated by Mr. Wallace's suggestion that infertility depends on "such a delicate adjustment" that it is more easily affected by isolation than some other adjustments. This is, I think, a very interesting point, as it suggests how it is that, in some cases at least, physiological divergence of this kind *is one of the first forms of divergence that arises*. But in some species other adjustments seem to be more delicate than this, and therefore more easily disturbed; while in others, several sets of adjustments, as colours and other recognition marks, with the preferences that correspond, and the habits of feeding and defence are in a state of equilibrium, the stability or instability of which is about the same as of that which determines the relations of the male and female elements. In this last class of cases, several forms of divergence may arise during the same stage of development, and that too when the isolated portions are exposed to the same environment. In some species a large number of characters are in a state of unstable adjustment. As Prof. Lankester has suggested near the close of his review of Mr. Wallace's book, this cause of divergence seems to be specially operative in the case of human faculties. But variability with plasticity of type is not the only condition that affects the stability of segregated portions of a species. Other things being equal, a single pair of any species is much less likely to represent the average of all the characters of the species than a million pairs. This consideration throws light on the comparative lack of divergence between the land animals of England and those of Ireland, which lack has been referred to by Mr. Wallace as an objection to my theory. In this case, many millions of some of the species were probably existing in each district at the time of the separation. As Prof. Lankester has pointed out, the representatives of the human species in the two districts have somewhat diverged; and the probability is, that if we were equally acquainted with the other species, we should find other examples of divergence in minor points. If the isolation is made more complete, and is longer continued, I believe the divergence will gradually become more apparent.

Mr. Wallace has mentioned another class of divergences which are best explained by the principle we are now considering—as he seems to have apprehended, though the process is not stated here as clearly as when discussing the divergences that produce infertility. The passage is as follows:—"The enormously lengthened plumes of the bird of paradise and of the peacock must be rather injurious than beneficial in the birds' ordinary life. The fact that they have been developed to so great an extent in a few species is an indication of such perfect adaptation to the conditions of existence, such complete success in the battle of life, that there is, in the adult male at all events, *a surplus of strength, vitality, and growth-power which is able to expend itself in this way without injury*. That such is the case is shown by the great abundance of most of the species which possess *these wonderful superfluities of plumage*. . . . Why, in allied species, the development of accessory plumes has taken different forms, we are unable to say, except that it may be due to that individual variability, which has served as the starting-point for so much that seems to us strange in form, or fantastic in colour, both in the animal and vegetable world" ("Darwinism," p. 293. The italics are mine).

It is no small gratification to me that Mr. Wallace has found this principle of unstable adjustment worthy of application to two important classes of divergences; and that, in the case of one of these classes, he has recognized that correspondence in such adjustments cannot be continuously maintained between the isolated portions of a species. I, moreover, have some hope that, when he understands the relation in which instability and isolation stand to each other in my theory, he will admit that it throws some light on the remarkable divergences of Sandwich Island land mollusks. The subject was only incidentally touched upon in my paper on "Divergent Evolution through Cumulative Segregation," but will be more fully discussed in a supplemental paper on "Intensive Segregation."

26 Concession, Osaka, Japan. JOHN T. GULICK.

Coral Reefs, Fossil and Recent.

MANY Alpine geologists believe the limestone and dolomite mountains which form so peculiarly beautiful and interesting a part of our Eastern Alps to be in great part composed of Triassic coral reefs. If this be so, their geological structure must necessarily contribute much towards elucidating the discussion concerning the origin of atolls and other forms of recent coral reefs. In this discussion, which has chiefly been carried on in England, the structure of our Triassic limestone mountains has been left out of account in a manner very surprising to me, considering that authorities like Richthofen and Mojsisovics have declared them to be remnants of coral reefs.

I have made a number of Alpine ascents in the dolomites of South Tyrol, chiefly in the districts of the Höhlensteinthal, Primiero, and the Langkofel, and have satisfied myself that the theory of the coralligenous origin of great part of these mountains is the only one which will explain the position and nature of the rocks composing them.

Not only do we observe in many places the massy dolomite alternating at its margin with sedimentary deep-sea deposit of

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